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Philosophical and practical considerations of the Braun-Blanquet system of phytosociology[1]

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Introduction

The subject of this symposium seems to me especially timely. Consciousness of the world's natural and seminatural vegetation has been accelerating at an increasing rate in recent years. This is true not only on the part of those men working in the basic and fundamental sciences, but also those active in the practical aspects where such "products" are involved as timber, forage, wildlife, water, soil and recreation. It is strange that, despite this situation, no single international scientific discipline has emerged which unites these many investigators, as we find in chemistry, physics and geology. Instead, we find an enormous number of unrelated techniques and methods. Groups of the investigators have often banded together and formed cults and "schools", with doctrines that are totally incompatible with those of other schools in the same field. Braun-Blanquet's system is one of these. This situation is not to be criticized adversely. It is indicative of an early growth stage of a science. It is incumbent upon all of us therefore that at intervals we should look about us to observe and adopt a greater maturity whenever it becomes available.

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[1]Paper presented in the symposium "Plant Sociology and the Taxonomy of Vegetation Units", December 30, 1952, 119th Meeting, American Association for the Advancement of Science, St. Louis, Missouri. This is number II in a series entitled Vegetation Science Concepts. Number I, "Initial floristic composition, a factor in old-field vegetation development", will appear in a coming issue of *Vegetatio*.

The author is indebted to many scientists for helpful comments on the manuscript of this paper, but especially is he indebted to Dr. Ruggero Tomaselli of the Università di Urbino, a leading Italian phytosociologist; and in this country to Dr. A. W. Küchler, University of Kansas, and to Dr. F. R. Fosberg, and Mlle. Marie-Hélène Sachet, both of the Pacific Science Board National Research Council.

The role of the United States in this field of science has possibly not been on a par with its roles in other sciences. Last year I expressed opinions on certain trends in American ecology. Besides the ecologists, there are many others interested in vegetation. Nevertheless, it is my personal opinion that there are only a handful of persons in this country whose chief scientific interest is the description and interpretation of its vegetation. As a type of proof of this assertion I could ask you to consider the titles of all the major American vegetation monographs of the last decade. With few exceptions, the titles reflect our strange preoccupation with an environmental determinism (to wit, "The relation of soil moisture to the vegetation of Mt. X"). Only rarely does one find a paper whose major expressed purpose is to describe the vegetation, regardless of what environmental factors are found to be correlated with it. The most costly and elaborate single vegetation project in this country is the survey of forest resources established by the McSweeney-McNary Act of 1928, under the jurisdiction of the U.S. Forest Service. An examination of any of the many "Releases" and maps that have been published as part of this project reveals the rather surprising situation that this Act has been construed as relating almost solely to the existing timber, timber types, and industrial statistics. Even the establishment of tree seedlings of the next generation takes a very subordinate role. The kind of survey of the forest types that would involve a more fundamental differentiation, including their history, their management, and their potentialities of conversion to different and more valuable ones—all this, certainly the foundation of a permanent silviculture—I find absent from the current work of the Survey, except for the unfortunately local influence of two individuals, Marinus Westveld in the Northeast (now retired), and A. E. Wieslander of California. The United States is not supplying a comprehensive science of vegetation.

The supporters of the Braun-Blanquet system of phytosociology (literally, plant-community science) are making a strong bid for its world acceptance. We all like to understand nature. And one of the easiest ways to "understand" nature is to disregard its complexities, and substitute a system or a theory which we can comprehend. In this instance, that which distinguishes Braun-Blanquet ideology is reducible to a relatively simple set of field and office procedures that can be grasped by, and are attractive to, relatively untrained technicians. It is clothed with a jargon that fools and flatters the unwary into thinking it is a scientific discipline of high intellectual order. It is strewn with pitfalls that allow for the materialization of subjective notions and preconceived ideas. Its published data can not, at least

in some instances, stand the tests of statistical checking. It is based on a philosophy which has never adequately been analyzed. It borrows concepts and methods from floristic taxonomy which are not only inapplicable but distortionary. I would be the first to claim that in the hands of masters it has added, and can add, very significant contributions to our scientific knowledge. Nevertheless, in regard to the considerations mentioned just above, I consider it the most dangerous single influence loose today, tending to regiment the field work, and to reduce the amount of independent thought that is of such extreme importance in a young and growing science.

Sources of Information

Sources of information for this paper come from reading and experience of the last 20 years. In the summer of 1936, an extended visit was made to the Station Internationale de Géobotanique Méditerranéene et Alpine (S.I.G.M.A.) at Montpellier, France. Specifically in the preparation of this paper, the following books and files of serials were consulted:

Beiträge zur geobotanischen Landesaufnahme. Zürich: Pflanzengeographische Kommission der Schweizerischen Naturforschenden Gesellschaft.

Bericht über das geobotanische Forschungsinstitut Rübel in Zürich.

Braun-Blanquet, J. Reprints of papers by him, in my library, and in that of the New York Botanical Garden.

Braun-Blanquet, J. 1951. Pflanzensoziologie, 2 ed. xi, 631 pp. Vienna: Springer.

Communications. Station Internationale de Géobotanique Méditerranéene et Alpine. Montpellier, France.

Drees, E. Meijer. 1951. Capita selecta from modern plant sociology and a design for rules of phytosociological nomenclature. Laporan balai panjelidikan kehutanan (Report of the Forest Research Institute) No. 52. 68 pp. Bogor, Indonesia.

Pavillard, J. 1935. Eléments de sociologie végétale (phytosociologie). 102 pp. Paris: Hermann.

Prodrome des groupements végétaux. Edited by J. Braun-Blanquet. Montpellier, France.

Rubel, Eduard. 1930. Pflanzengesellschaften der Erde. viii, 464 pp. Berlin: Huber.

Vegetatio, Acta Geobotanica. Organe officiel de l'Association Internationale de Phytosociologie. Den Haag: W. Junk.

Definition of Terms

The wording of the title of this paper was not chosen by me. When I first saw it I disliked it, believing it implied something that was most alien to my own way of thinking. Later, I realized it expressed that thinking precisely! How both? Because of two meanings of that otherwise innocuous word "and". The "and" of "black and white" is not that of "leal and loyal". In one case it connects antonyms; in the other case it connects synonyms. The synonymic use is here intended. Through all my scientific work I find that so-called philosophic aspects are part and parcel of both practical and academic activities. Furthermore I have never been able intellectually to separate the practical and the academic—except in terms of what at the moment may give one either an income or an unnecessary gadget.

The term "plant sociology" now demands even further delimitation than that given it by the preceding speakers. The term is fraught with semantic confusions that have prevented the full utilization of the literature. The first distinction in concepts, that between "flora" and "vegetation", has been repeated innumerable times in the European literature, and is the basis of almost their entire thinking and much of that of the rest of the world. We can pass over it now by saying that studies of "flora" involve species as isolated units in nature, without regard to their aggregation in varying degrees of abundance, whereas "vegetation" considers the species only as the building block in forming a mosaic or tapestry of plant communities on the earth's surface. Braun-Blanquet's work is in this latter category.

The second distinction will be more strange to you, but it is an integral part of the line of thought I wish to follow. I would like to divide that which I above called "vegetation" into two ideas, differing in their level of integration, in the inclusiveness of their subject matter. By analogy, the study of isolated cities is an approach on a different "level" from the study of states each composed of numerous inter-related cities. In the first case—to return to botany—we have a science strictly of plant communities (cities), described, named and classified and otherwise handled as though they are the prime units in nature. This, and only this, is the "'plant sociology" of the distinctive Braun-Blanquet ideology. In the second case we have a science which considers these communities only as the building blocks composing larger complexes of "vegetation", which are complexly united in space and time. There are probably more workers in the world who are involved with these larger concepts of what might be called a

"vegetation science". If this distinction sounds unreasonably hair-splitting, I ask you to look at Braun-Blanquet's "Ubersicht der Pflanzengesellschaften Rätiens" (in Vegetatio, volumes I and II). In this flawlessly orderly classification and description of the numerous communities of this part of Switzerland, one struggles in vain to build from the data a picture of the vegetation of Rhaetia, of which communities are most abundant and dominate the landscape, of how they are correlated with the major topographic and edaphic features, and of how they are replaced by other communities on disturbance, or will develop to others on lack of disturbance. A science of "plant sociology" is valid; but let us not confuse it with what we may consider a larger, more inclusive, "science of vegetation". The difference is very real and tangible.

There are large numbers in Europe who are doing excellent research in the study both of plant communities and of vegetation. Many of these men call themselves "plant sociologists". For my present purposes, I wish to separate this desirably amorphous group of active workers ("European plant sociology" in a general sense). Some of these men would call themselves followers of Braun-Blanquet, and yet it is my contention that they have so modified the basic code, so cut away in some places, and added onto in others, that I cannot include them in this discussion. It is not "European plant sociology" which I am criticizing adversely.

And now for an odd semantic twist: As recently as 1952, a paper has appeared in an American journal entitled "A phytosociological analysis of . . . . " There has been a long train of such papers in recent years. Is this the phytosociology of Braun-Blanquet? Absolutely no, even tho in some instances the author so intends. This American usage of "plant sociology" represents a curious error that I really feel we should now recognize. By example, we may use our two hands. Let the right hand represent plant sociology in Europe (or vegetation science–the distinction is not here relevant). Each finger represents a subdivision of the science: one for its floristic composition, one for its form and structure, one for space relations, one for time, or whatever other type of subdivision you wish. Let the left hand represent the study of vegetation in America – under any name. Each finger represents the same subdivision of the general field as decided for the other hand. Now it so happens that one of our American fingers was extremely poorly developed, almost not existent, that dealing with the form and structure of communities. The same finger on the European hand was very fully developed (though by no means developed to the detriment of the others, as

some Americans in their ignorance of the literature would have us believe). George Nichols and Stanley A. Cain both brought over this continental European form-and-structure finger, but the titles of their papers were not carefully read. We proceeded to graft this finger onto our own deformed hand. The linguistic oddity is that we called the *finger* "plant sociology", not realizing that that name belonged to the *whole* European *hand*. It is difficult to understand how this bit of sheer foolishness continued among a few of us, even with Braun-Blanquet's textbook translated into English, but at least it is not too late to rectify what even etymologically never was reasonable.

At this time, I cannot refrain from pointing out a comparable semantic oddity. Again referring to the hands and fingers: one of the European fingers, that referring to the environmental relations, is designated by them, and very logically, as "ecology". But the same finger on the American hand has no name, while the *entire* hand is called "ecology".

This strange situation, of two terms, each being used both for a part and for a whole, representing two different parts but only one whole, has caused endless confusion between English-speaking, and non-English-speaking groups. And I regret to say that the weight of etymologic and historic reasonableness is not with the former.

Braun-Blanquet's phytosociology circumscribed

To comment upon Braun-Blanquet's system of phytosociology, it is necessary to so circumscribe it that its individual and particular features are spotlighted, without including those that are common to other systems. On these grounds, a major part of Braun-Blanquet's textbook (1951) is excluded from the present discussion. Actually, this volume is one of the best in the field. With 45 percent of the text devoted to discussions of environmental analyses, it might be considered by some to be overweighted on ecologic aspects!

Historical origins are beyond the scope of this paper. Without entering upon details, it may be said that the germinal ideas did not arise with Braun-Blanquet himself. They antedate him and may be traced to Rübel, Brockmann-Jerosch, to Du Rietz, Moss, Gams, Cajander, Schouw, and others. Most of the ideas were presented soon after 1900. By 1920, Braun-Blanquet had molded them in much the form they now occupy. In 1925, Braun-Blanquet still had his academic affiliation in Zürich, and his system developed within the influence of the scientists there. By 1930, the Station Internationale de Géobotanique Méditerranéene et Alpine was well established at Mont-

pellier in southern France, and that station has since been the headquarters of this group.

This "school" is referred to by its followers as the "Zürich-Montpellier School". The name is unfortunate, for it has long since ceased to be typical or representative of either city. Zürich is the seat of the Geobotanische Forschungsinstitut Rübel. This organization now uses, and was the prior user of, many things that we associate with Braun-Blanquet, such as the terminations of -etum, -ion, and -alia, and the concepts of Constancy, Presence, Frequency, et al. On the other hand, Rübel's system uses physiognomy in the classification of vegetation, and in this and other respects, the two schools are totally divergent. Even "Montpellier" is not a distinctive epithet, for the city is also the seat of the Ecole Nationale d'Agriculture, where J. Kuhnholtz-Lordat and his students are actively engaged in vegetation studies that have little relation to those of Braun-Blanquet. Distinctively, the ideas of Braun-Blanquet and his many students in foreign countries are centered in the S.I.G.M.A., and when in reference to those concepts and ideas I shall use the word Sigmatism.

As is true of any school, when one reads enough of its literature, one soon finds such wide variation, such overlapping and interlocking with the ideas and methods of other groups, that the individuality all but ceases. In the comments that follow, I have tried to avoid discussing these marginal forays into other territories, and shall restrict my consideration to that which I consider relatively pure Sigmatism.

It might be appropriate to mention here that the tenets of a "school" no matter how restricted, can, in the hands of any master, become so pliable and of such potential effectiveness that they far transcend that which the ordinary worker accomplishes. This has happened with Braun-Blanquet's group. There are several men— R. Tüxen, to mention but one—who are enthusiastic advocates. I am willing to risk the statement that these individuals would have accomplished the same results had they started with a different "school", even with Clementsianism, to use another extreme. It is curious that in the hands of such masters, the differences between the schools seem to lose their significance. Rather, one might say that under such circumstances the underlying universality of all schools becomes dominant. I would like to think that this condition is a harbinger of future unity.

It is believed by many that the characteristic features of the Braun-Blanquet system lie in his terminology of association, and in the structural concepts of Frequency, Presence, etc. To the contrary, these are *not* part of its most distinctive qualities. It has already been

mentioned that the Geobot. Forsch-Inst. Rübel at Zürich uses these techniques. Furthermore, the idea of analyzing vegetation quantitatively by means of small quadrats has arisen spontaneously many times in different countries, and there are countless systems of expressing the quantitative abundance of species in the total cover. Many of these are equally good if not better than the particular methods used by SIGMA, but they lack the dignity of formalized cut-and-dried presentation.

We find that Sigmatism, as with any school, has been plastic in the hands of some scientists, and has thrown out branches in many directions. It is not easy to delimit it, but we find that, regardless, it does encompass certain features which collectively form a highly distinctive and characteristic body of scientific thought and method.

Sigmatism: its characteristic features

1. *Species Identification.* One of the most impressive features of the Sigma publications are the long lists of species. Not only do these lists portray the floristic composition of the Associations, but they are of the utmost importance in the classification of those Associations. This aspect of the work implies a most thorough grounding in practical plant identification. In this regard, Braun-Blanquet and a long list of other workers reveal a familiarity with floras which many of our own American workers do not emulate, including an acquaintance with modern cytology and genetics, and their implications to taxonomy. On the other hand, the mere need of a name in a published list is an open invitation for careless identification. When the reader scans thes lists, he constantly finds binomials, often in significant categories as Character Species or Differential Species, representing plant groups of taxonomic complexity, where the identity is at best a subjective judgment, or where we feel that the opinion of an authority in taxonomy should have been asked. There is indirect evidence that herbarium collections are sometimes made. Yet it is here that we find one of the weakest joints in the entire Sigma structure. Nowhere is adequate emphasis placed upon the making of complete collections of plants, for permanent deposit and to serve as vouchers that may be checked by future investigators. Indeed, this procedure is technologically impossible within the system, for single quadrats are often taken at widely scattered intervals in time and space, and taking a full set of floristic vouchers would increase the man-hours involved many fold. Thus we have the anomaly that in a phytosociological system squarely based upon floristic composition, that composition usually must be taken on faith of the reader, very often

under circumstances that lead us critically to wonder at the very impressive floristic knowledge displayed.

2. *The concept of the species.* Another joint that shows a grave weakness—and this time in a most critical position for the whole structure—is in the concept that lies back of the use of a binomial (or in some instances, trinomial). These named entities are at the foundation not only of the identity of the Association, but of its nomenclature, and of its classification into higher phytosociologic units. It may come as a surprise therefore when we realize that the only kind of species to fit this category is the outmoded and discarded "Special Creation species", one which is consistent and uniform in every individual, and which possesses similar physiological and ecological relations throughout its range. To be sure, allowances are made for the exceptional and impossible-to-ignore situations such as our own Douglas fir and lodgepole pine would create, both of which grow in several totally different types of vegetation. Sigmatism however exists on the assumption that the named taxad is vegetationally consistent, homogeneous, and reliably uniform in its associational relationships. I fully grant that many species may show such general affinities, but whether they do so in terms of individual small quadrats is a very different phenomenon. In my opinion, Sigmatism cannot exist if we are to accept the current concept of a binomial as covering a population of individuals that are genetically variable and still varying, comprising local colonies and ecologic races, and with opportunities for hybridization, gene-flow and introgression, either on infra- or inter-binomial levels, and capable of advancing into areas and niches not now occupied.

3. *Collecting Field Data.* In the Braun-Blanquet system, the taking of field data is—as with any other system—as good or as bad as the man who takes it. I rather suspect that this system allows more opportunities than others for personal bias, without its detection by other scientists. If one wishes to understand this aspect of Sigmatism, he has only to transfer the philosophy of the field-collecting of the floristic taxonomist, who for decades has roamed the forests and grasslands, looking for different populations, and picking those samples that he considers representative. That he is tempted to put undue emphasis on the abnormal, the strange, the "hybrid", the unusual, is only human. Phytosociologists can collect *relevés* and *Aufnahmen* (quadrat analyses) with the same gay reckless abandon, not only on specially arranged expeditions, but while on summer picnics, at railroad stations while the locomotive is chuffing, at stops along the highway, always with a keep appreciation for the unique

and the rare. All these samples of nature are dragged back to the office; and the rest of nature is forgotten; each is pawed over intently, perhaps years later, and eventually is relegated to a named category of "reality". This system has inherent in it all the flaws and gross errors that have made some of our herbaria depositories for abnormal plants, rather than representative populations.

4. *The Nature of the Association.* Agreement as to the nature of the Association has always been a point of dissension among phytosociologists. Past botanical congresses thought the problem could be solved by agreement on a verbal definition. But when the verbal definition was translated into phenomena in nature, we still found the same divergences. The difficulty lies in the fact that there is no agreement as to the underlying concept of the Association, or even awareness as to what concept we each have. In the Braun-Blanquet system, this concept is unique and distinctive, and to try to equate it with others, as by listings in parallel columns, is sheer folly. It is true that in some instances a single specific concrete Association in the field may be the same as that recognized by some other concept. The concepts of Association are not thereby the same. SIGMA Associations are established by a purely subjective visual comparison of tabulated quadrat data. The key feature is the presence or absence of so-called Characteristic Species, and Differential Species, quite independently of their abundance. In short, the Association is created on the grounds of an observable repetition of certain floristic elements, as found in from one to many *relevés* that may be widely separated in space and time. Unless one adheres to this concept, one does not follow the SIGMA school. I do not wish to criticize the concept unjustly. It is closely related to the "indicator species" of other authors. It is a product of rational thought; it is an excellent working scientific hypothesis; it deserves to be tested on statistical grounds and on the basis of extensive field observations. It presupposes the ecological homogeneity of binomially designated taxads. It also presupposes a constant linkage between species in nature, even on the geographic basis of a one-meter-square quadrat. It is my personal opinion that this Association is not the unit of nature which most often I myself wish to recognize. It is true that all sorts of modifications and variations can be established, to accord with what are our subjective feelings and our "common sense" on the matter. But then we end up with an elaborate system of exceptions and a complex terminology that destroy the very essence of the concept.

In the world at large, the elements of chance and coincidence in floristic distribution are being recognized more and more in the

empirical study of vegetation types. It is interesting to reflect that since the presence or absence of species is most critical in the establishment of Associations, and in their classification, Sigmatism cannot admit the existence of chance. If it is admitted, then one makes a subjective exception which strikes at the very heart of the entire system, and opens it to other exceptions whenever the whim of the investigator dictates.

I confess I long thought that in the impressive Association tables, the segregations into groups classed as Characteristic, Differential, Indifferent and Accidental were determined on mathematical grounds from the basic quadrat data. To the contrary, these are determined by subjective evaluation. We can anticipate from D. W. Goodall (recently of the University of Melbourne, now of University College of the Gold Coast) a paper now in advanced state of preparation,[1] in which he has applied statistical techniques to such published tables. He finds cases where these published data do not justify the designation of many species as Characteristic. In fact, there are instances where a species regarded as characteristic actually has a higher presence value in another Association! Nevertheless, this criticism, as Goodall himself readily states, does not topple the SIGMA System. We should remember that even if the organization of *relevé* data into Association tables is in part subjective, the very choice of those *relevés* was as illustrations of conclusions determined upon other grounds, which one is asked to take on trust, as representing the impartiality, the skill and the field experience of the author.

5. *Variation within Associations.* Superficial inspection of the impressive published Association tables may lead to a comparison disparaging to other phytosociological systems based on assumedly fewer data. But on closer observation, we find that each vertical column represents the analysis of but a single quadrat! An entire Association may be described on the basis of but three quadrats, and theoretically but one. When we realize that there is no way of telling whether these three quadrats were chosen after years of acquaintance with the vegetation, or after a day's intensive local field work, or less, then we realize the tremendously variable element in these publications.

To those field botanists who take for granted the sampling procedures involved in modern statistical analyses, it may come as a complete surprise that Sigmatism has made no allowance for what,

[1]Since published. Goodall, D. W. 1953. Objective methods for the classification of vegetation. I. The use of positive interspecific correlation. Australian Jour. Bot. 1 (1): 39-63.

in floristic taxonomy, is referred to as mass-collecting! The *Aufnahme* is subjectively chosen as representative of the Association, and only one might be taken in a large geographical territory. For this reason, the actual number of man-hours in field and office represented respectively by some of the Sigmatic Association tables, and by such studies as those of J. T. Curtis, are in such totally different categories as to be incomparable. Moreover, all the logic and thought of modern statistics, with its analyses of multiple samplings, is missing from work of SIGMA.

That the Braun-Blanquet school had not given adequate attention to a description of variability within Associations was recognized by Drees, who has suggested a large number of subdivisions and categories, replete with a complex terminology that will please those who like such complicated systems. It is only one step from these numerous categories of variation to the idea of continuously varying vegetation, which Curtis and Whittaker have both been describing by newer statistical techniques. Let us not forget however that such a continuum is by no means omnipresent in vegetation. Neither is a statistical treatment more than an adjunct to *description;* it does not provide an *interpretation*. Neither is such a continuum a new idea. The constant variations between and among plant communities are recognized in a literature-continuum in this country from the start of the century. The idea of discrete knife-edged associations is actually found in a minority of the publications, and is due to an oversimplification on the part of a few authors, which has been still further simplified by some readers.

It is appropriate to mention at this time that whether vegetation is composed of discrete non-integrating Associations, or whether it is an ever-varying continuum may be more a matter of man's method, than of nature. To separate things into discrete and non-merging parts is perhaps the more primitive in the history of science. The treating of merging phenomena (transitions, ecotones, or continua, as the fashion determines) often comes later. Probably our intellectual heritage as individual animals dictates our thoughts. Had we been sponges, colonial protozoans, or social bees, the science of phytosociology might be very different! In regard to our scientific methods and what they "prove", we can learn much from the old arguments on the nature of light. Tests were devised that absolutely "proved" that light was particulate in nature. Other tests were run to "prove" that light was a type of wave radiation. All these tests reflected the mind of man as well as the nature of Nature. The ultimate conclusion was forced on us that light was an unknown, that it could react

in both these ways. I venture to say that discrete plant "associations" must invariably appear when the test of Characteristic species is applied to *relevé* data: the method creates the Association. Comparably, there are other tests which, when applied to any vegetation must necessarily and unavoidably, "prove" that a continuum exists. It would for example be very easy to establish the "fact"—if I may be excused a *reductio ad absurdum*—that a monkey is a continuum of bone, blood, muscle, and other cells and tissues, rather than of definite anatomic structure. If he were frozen, and sliced into thin segments, like a loaf of bread, and each segment ground up, then the analyses of the minced monkey meat, segment by segment (a 100% sample), would prove beyond a doubt that the materials analyzed for do exist in a varying series, with no sharp discontinuities. It is true we might, on good statistical grounds, eliminate the head, for with all its brain tissue, it is obviously so different from the rest as not to be part of the series. Thus, as botanists,, we would be tempted to eliminate a pine stand if it occurs in a deciduous forest region. Or we might eliminate the top of the skull, for sheer difficulty of sawing through it. Similarly, I myself would be sorely tempted to eliminate (and I know of some who have eliminated) the forest stand that was choked with tall blackberries. In short, the most refined of statistical techniques rest upon certain subjective and philosophic bases which do not detract from the method, but which should be considered part of it.

6. *Nomenclature and taxonomy of Associations.* A totally different problem is raised in the consideration of the segregation and naming of associations and higher units, and of their arrangement in a hierarchical classification. Just as the physicists, in their investigation of light, tried to identify it with known phenomena (particles, or waves), so the SIGMA phytosociologists have identified the plant community with the binominally named population of plants. A working analogy is an excellent scientific device, and I do not criticize it adversely. In this country we started on the analogy that the community was like unto an individual organism. That was helpful; but it became ridiculous when the analogy became an identity and we reasoned that since the organism possessed certain attributes, the community must have them also. With the Braun-Blanquet school I find no conscious acknowledgment of this philosophic analogy to populations of individuals. I seriously wonder whether the analogy has not unobtrusively become an identity.

In the field of nomenclature, the problem is especially evident. Most recently Drees has presented a 30-page proposal, replete with rules for forming names, matters of priority for the author's name

(which follows the name of the community as in floristic taxonomy), types, divisions, recombinations, the use of "nova", nomina conservanda, and all the other paraphernalia, openly modeled after the International Rules for Botanical Nomenclature for floristic units. Aside from the fact that their abuse of the Latin language makes our Roman scholars froth at the mouth, it is a serious question whether or not plant communities are sufficiently similar to floristic taxads to warrant taking over bodily the nomenclature code developed for them.

I have one special qualm in this naming game, especially as it relates to the absolute simplicity with which new Associations and higher units may be created, to bear for evermore their authors' names. I do not imply that any phytosociologist now active has unnecessarily created such a named taxad. But one shudders at the horrific thought of the pounds of printers ink and the tons of paper pulp that will be expended when the emotionally dwarfed foot-soldiers (in the front ranks of every science-army) discover this apparently safe and harmless path to phytosociologic immortality. Witness the state of *Hieracium* and a host of other genera! With the experience of the plant taxonomists before us, it is to be thought that if there are mature individuals amongst us, we should at once plan to curb such immature urges in others, for these individuals can seriously complicate and hinder the advance of the entire science.

This analogy of communities to populations is strikingly evident in the matter of classification of the Associations into higher units. In the Prodromus, and in the conspectus of the communities of Rhaetia (in Switzerland), one might, at the first quick glance, mistake the printed page for one from a floristic taxonomy manual. Here are names and descriptions comparable to families, genera and species, each with concise diagnostic descriptions, with all the conventions we are accustomed to see. This system is the inevitable result of accepting the dogma that floristic composition is *the* criterion for relationships between communities. Those Associations which have Characteristic Species in common are united into one Alliance. On the next higher level, several Alliances which have certain species in common are united into one Order. And so on. The Association-Characteristic species, the Alliance-Characteristic species, and the Order-Characteristic species may or may not be the same plants, with the result that subjective decisions are more and more easily made by the socio-taxonomist. Furthermore, nowhere is it made clear just how much or what floristic similarity is necessary for the establishment of a group at any one level. Whereas in the taxonomy of plant species, the factor of phylogenetic relationship is a rational and

emphatic support for the hierarchical classification we use, in the field of plant sociology, the analogous classificatory factors are these subjectively determined Characteristic Species. I seriously doubt that the phenomenon is worth the scientific weight attributed to it by this analogy-become-identity.

F. R. Fosberg called my attention to another aspect of the problem which is overlooked by its very obviousness, and that is the fact that classification must proceed from the smaller to the larger unit, and never in the reverse order. Scientifically, it becomes impossible to recognize the unity of the spruce-fir forest, *until* one has laboriously worked his way up from all the little -eta. This thought logically leads to a very surprising conclusion. It is observable that all the larger Circles and Classes of vegetation are essentially those which other vegetationists agree upon, and have derived by different and incompatible techniques. That the larger SIGMA groups are correlated with those of other workers means either that the phenomenon of Characteristic Species *is* a fundamental objective scientific fact comprehensible only to the specially initiated, or else that there has been a conscious or unconscious influence to so choose the Characteristic Species as to obtain the larger units which we subjectively "know" are reasonable ones.

Actually, there are manifold, multidimensional relationships between communities, several of which may be important for different purposes. Relationships in space or in time may locally be most important. Matters of general physiognomy of a predominant species (never important in Sigmatism), or of management and cultural techniques, may elsewhere apply. An acceptable taxonomy is one based on the use of classificatory factors which are correlated with the greatest number of other attributes. In vegetation science it seems that we should recognize several taxonomies, (and Dr. Braun-Blanquet would be the first to say that others can be found in his writings).

In facing Sigmatism, one faces that fundamental facet that fronts each follower: *Is* the existence of Characteristic species as shown from *relevé* data (which may be quite rare in terms of number of individuals) of such basic importance throughout nature that it may be made the basis of a world wide System? Granting that its use is "logical", does it not show relationships of little if any significance? And does it not cut across relationships of considerable value? One cannot avoid the admission that the extreme similarity between the concepts and techniques of such aspects of Sigmatism and of floristic plant taxonomy can lead to the accusation that the latter is molded in the image of the former, without original or subsequent considera-

tion as to whether that image is philosophically, academically and practically a suitable analogue.

In conclusion, I would like to say that the Braun-Blanquet system is a noble scientific experiment. It is one of several logical and rational approaches that mankind sooner or later would have had to take. In that respect, it is like a non-Euclidean geometry, entirely logical—but whether it fits the world of experience immediately about us must be decided separately. This Sigmatism is one of the most significant milestones in the history of vegetation science. Through its years, it has been the vehicle for a quantity and quality of phytosociologic investigations which transcend what many other countries have produced. But the greatest danger of disintegration lies in the opportunity of oversimplification. Like its preceding Clementsian cult, it is philosophically simple, and now stands in danger of becoming a stereotyped orthodoxy, an outmoded classicism, codified, regimented, and standardized; "closed' in the worst sense of the term. It may well attain considerable popularity, however. It is flattering in that it allows one of relatively little training to take *relevés,* name Associations, and publish papers. While deluging us with "facts", it may lull us into a false sense of security. While satisfying our urge for "original research", we may lose sight of the more important goal: a closer approximation to the fundamental nature of vegetation, and to the attainment of a universal vegetation science.

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK

Discussion of a Portion of the Ulotrichaceae[1]

HERMAN SILVA FOREST

Ulothrix and genera which have been considered closely related to it have received attention in the study reported here. Of this group, *Ulothrix* Kützing (1833) is probably the most familiar, not only to investigators in the field, but also to many students in elementary botany courses, who know it as a plant illustrating reproduction by biflagellate gametes. In the family Ulotrichaceae, genera considered to be closely related to *Ulothrix* are *Hormidium* Kützing (1843), *Stichococcus* Naegeli (1847), *Geminella* Turpin (1828), and *Uronema* Lagerheim (1887).

Gay (1891) and Klebs (1896) extended our knowledge of the group in the last century, and the treatment of Heering (1914) might be considered the standard modern European system for the whole family. America studies of the Ulotrichaceae have been based chiefly on the conclusions of Hazen (1902), although Smith (1933 and 1950) has made some revisions.

The difficulties experienced in applying both the European and American systems to the task of identifying living material encountered in 1949-50 studies of the algae in the Tennessee Region prompted investigation of the group, and results are reported here.

In order to broaden the base for the study beyond the material available in the area amounting to only 40 collections, about 500 dried specimens were borrowed from the New York Botanical Garden and the Chicago Natural History Museum.[2] These specimens had been collected from a wide geographic and ecologic range of sites, and included material identified by Kützing, Hansgrig, Rabenhorst, and Hazen, among others. The preservation was usually good, the oldest specimens being in almost as good condition as more recently collected material.

In order to establish a uniform basis of comparison between many individuals, certain characteristics were observed and recorded. These characteristics had all been employed by other workers to delineate genera and species. They included cell diameter, relative cell length, cell wall thickness, cell tumidity, the nature of the chloroplast, and reproduction.

The result sought here was the segregation of entities in the population, the plants themselves dictating their own classification.

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[1]Contribution from the Botanical Laboratory. The University of Tennessee. N.S. No. 141.

[2]The writer is indebted to Dr. D. P. Rogers and Dr. Francis Drouet, respectively for these loans.

No special attention was given to the establishment of the validity of the names used, either as to the identity of the type specimens on which they are based, or to the priority of the names. Nomenclature was considered to be a problem separate from such a systematic study, although one which should logically follow.

1. Diagnostic Characters

As can be observed in several groups of flowering plants, as well as the algae, the comparison of a large number of individuals often tends to demolish the notion that all specimens can be fitted into neat pigeonholes of previously established genera and species. Mass observation indicates that complex variable populations make up many groups, and the Ulotrichaceae appears to be such a group.

It was not unexpected then, that some criteria of differentiation applicable to a few specimens should prove to be of limited or no value when many were observed.

Variation in cell wall thickness and *cell tumidity* were soon perceived as results of age and environmental conditions, and all transitional stages were observed in cells of single filaments. These characteristics were consequently dismissed from consideration as useful diagnostic ones.

Relative cell length appeared to be of little reliability in the separation of entities, although it has been employed as a character in most keys and descriptions. There were two cases in which entities seemed to be recognizable partly by their cell length, and the character is considered of value in those cases, at least until further evidence is obtained to confirm or reject its validity. *Filament length* was useless as a means of separating entities, although some of the smaller forms do disassociate more readily than do the larger ones.

Pyrenoids have been used to separate genera by Heering (1914), and Smith (1950), but the visibility of this structure varies within a single filament. A single pyrenoid or none is visible in small filaments, and few to several in larger ones. In only one species was the pyrenoid distinctive enough to be of possible critical value.

Cell diameter and *nature of the chloroplast* were finally utilized as the chief diagnostic characters, although filament tips, holdfasts, pyrenoids, and cell outgrowths were of value in certain cases.

Natural groupings were detected in cell diameter measurements, although there were few actual gaps in size between groupings. Within groups most plants were similar or intergrading in size, but some plants showed a wide variation in size that placed them in no group (Table A), and others were of borderline size, between groups.

Table A. Specimens of broad or unusual latitude in variation of filament diameters

latitude	frequency (out of 500 specimens)
4-8μ	1
5-9μ	4
5-10μ	3
5-11μ	1
7-10μ	6
7-11μ	6
9-13μ	1
10-14μ	6
10-20μ	1
11-18μ	3
12-15μ	1
13-25μ	1
17-25μ	1

Hence, the natural groupings selected from observations are not completely separable, and are divided somewhat arbitrarily, for convenience in classification.

The ulotrichacean chloroplast is a parietal plate, usually with one or more pyrenoids. Virtually all authorities of this century (vide Hazen, 1902; Heering, 1914; Smith, 1933 & 1950; and Prescott, 1951) differentiate between the "*Ulothrix* and the "*Stichococcus*" and/or "*Hormidium*" types. The *Ulothrix* type is described as an almost complete ring, while the others are called plates, circling less than half the cell perimeter. Rings were found only in large specimens, very small plates only in small specimens, and the types intergraded most frequently among mid-range specimens. Single filaments displayed variation through the range from a small parietal plate, occupying about a quarter of the visible cell surface, to a broad "double folded" plate circling about two thirds of the cell circumference. Variation found in different individual filaments overlap in chain-like fashion throughout the complete range of types.

2. Grouping and Nomenclature

Geminella and the marine *Ulothrix* species have been omitted from this discussion, inasmuch as they appeared as distinct and did not present complexities of the magnitude encountered in the fresh-water population of *Ulothrix* (including *Hormidium, Stichococcus,* and *Uronema).* Brief comment is given them under Conclusions, below.

The fresh water population has been divided into categories simply called groups, as a means of stating their sometimes heterogeneous, and certainly provisional, nature In all groups a representative species name has been selected in order that the system may be utilized. It should be understood, however, that the species concepts employed here do not correspond directly to the original ones.

Group A—

The smallest individuals have been listed under the names of *Stichococcus bacillaris* Naeg. (ex Hazen), *Hormidium pseudostichococcus* Heering, and *Ulothrix limnetica* Lemm. (ex Heering). The first two are differentiated by Heering only through the pyrenoid attributed to *Hormidium*, and both are 2.5-3μ in diameter. The *Ulothrix* in this group is too uncertainly characterized to be evaluated. The culture (Pringsheim's isolation) of *Hormidium nitens* Menegh. examined definitely belongs here.

In the population of the group, a single, slightly variable entity, corresponding most closely to the general concept of *Ulothrix* **bacillaris** (Naeg.) comb. nov. (Fig. 1),[3] appears to be present. Its filaments readily disassociate into single cells of oval or oblong shape, especially in non-aquatic habitats. The chloroplast is usually a plate appearing to occupy most of the visible cell, but it may be a small lump. A pyrenoid may or may not be visible in it. The filaments are 2.5-3.5μ in diameter, and the cell length is 1-3 times its diameter. This plant grows quite well in terrestrial sites and is occasionally found in water. Reproduction by motile cells is not known.

Group B—

Only a few individuals appeared in this group, but there were enough to make it appear distinct at this time. It should be fully understood that the exact relationship of the group is most uncertain since it not only shows affinities with *Stigeoclonium*, but also may grade imperceptibly into *Ulothrix* groups C and D. The filaments are about 4μ in diameter. Described species of this size are: *Ulothrix subtilissima* Rab. (ex Heering, 4-5μ), *Stichococcus scopulinus* Hazen (3.5-5μ), *Ulothrix limnetica* Lemm. (ex Heering, 4μ, and var. *minor* 2μ). *Ulothrix subtilissima* (Fig. 2) is taken here to represent the group best.

The individuals of this group were aquatic with pointed end cells, and holdfast not differing greatly from normal vegetative cells.

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[3] *Stichococcus bacillaris* Naeg. of Naegeli, C. W. 1847. Gattungen einzelligen Algen, physiologische und systematische bearbeit. Zurich.

The chloroplast is frequently lumped along one side leaving over half the cell clear, but it may be broader and flattened or apparently folded on one, rarely both, sides. The chloroplast is yellowish in microscopic view, but the plant is quite green to the unaided eye. A pyrenoid is generally visible. The cells appear to be consistently long. 1.25-5 times their diameter in length. Reproduction by motile cells is likely, but direct evidence is lacking.

Group C—

There are in herbaria a great number of *Ulothrix*-like plants which measure about 5-6µ in diameter. Within this group the most common report is of *Stichococcus subtilis* (Kütz.) Kleck.[4] (ex Hazen, 5-6µ) (ex Heering, 5-7µ or more, with modified characters), and *Ulothrix variabilis* (Kütz.) Kütz. is sometimes identified. Possibly *Hormidum Klebsii* G. M. Smith belongs here too.

In spite of the liberal representation of this group in herbaria, it has not appeared in a subsequent search of field material, so the possibility appears that the group does not exist except in herbaria, and all individuals assigned to it actually belong in the following group.

Both aquatic and terrestrial plants have been assigned here. The chloroplast may be lumped along one side, assume an hour-glass shape, occupy the visible cell surface with a flattened plate, or, occasionally, it is folded on one or both sides. The color is quite green. A single pyrenoid is generally visible. The cells are 1-3 times their diameter in length. Reproduction by motile cells is infrequent or absent. Reproduction is evidently by fragmentation of ordinary filaments and by modified vegetative cells. Pointed filaments are infrequent or absent.

Group D—

This group was not frequently recognized in herbarium materials seen, but appeared in several cold water sites during this study, some times in enormous quantity. *Ulothrix rivularis* Kütz.[5] (Fig. 5) typifies the group's characters best, but *Stichococcus fluitans* Gay could not be separated. *Ulothrix subtilis* Kütz.[6] and *Ulothrix subtilissima* Rab. might represent the smaller filaments of the group, which do not exhibit the full range of diameter variability found. The group's diameter size range is characteristically broad, certainly 5-10µ.

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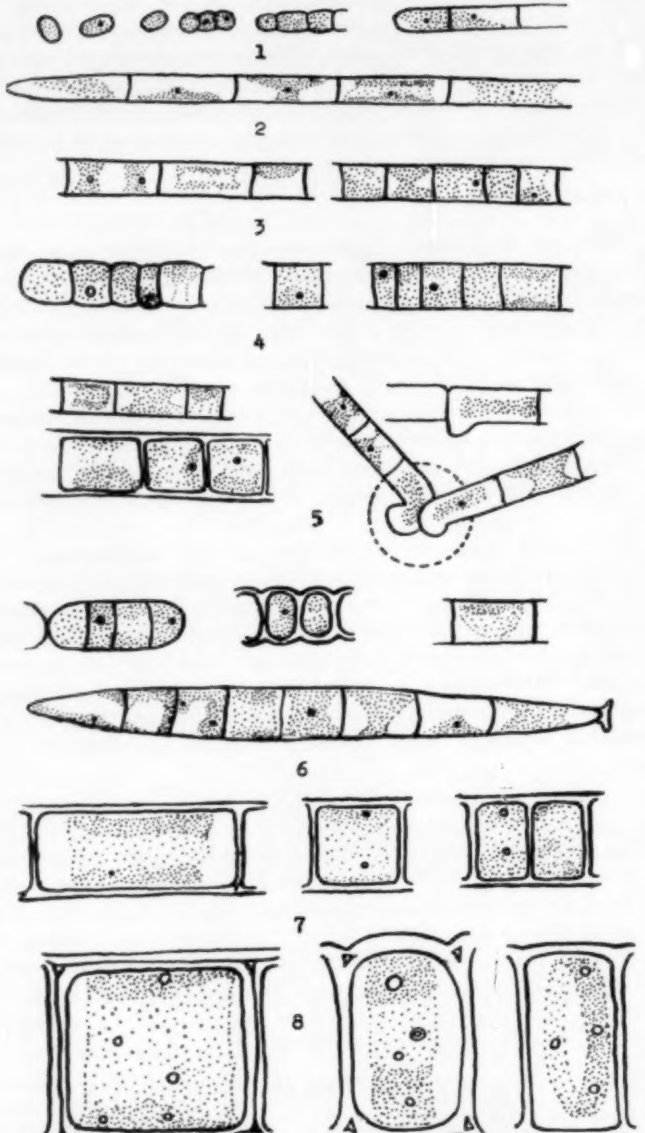
[4] *Ulothrix subtilis* Kütz. (Fig. 3) or *Hormidium subtile* Kütz.) Heering.

[5] *Stichococcus rivularis* (Kütz.) Gay (8-11µ, ex Hazen) and *Hormidum rivulare* Kütz. (4-11µ, ex Heering.

[6] *Stichococcus subtilis* (Kütz) Kleck. and *Hormidium subtile* (Kütz.) Heering.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Only aquatic, mostly cold water specimens among the fresh material observed were placed here. The chloroplast in fresh specimens is similar to, or identical with, that described for the previous group. It may be lumped along one side, hour-glass shaped, a flattened plate, or, occasionally, folded. In shorter cells, usually more of the surface will appear occupied by the chloroplast than in longer ones. A single pyrenoid is generally visible. The cells are 1.25-3.5 times longer than their diameters. Reproduction by motile cells is infrequent or absent. Reproduction is evidently by fragmentation of ordinary vegetative filaments and by modified vegetative cells, and sharp-ended filaments are infrequent or absent. The "rhizoidal" outgrowths described by Hazen and others are not always present, and cannot serve as the sole criterion for separation. Plants have been observed, otherwise indistinguishable from typical filaments, which have a few long tapering branches of one or two cells. While these forms were referred to *Stigeoclonium*, the degree of closure between the two populations at this point is notable.

Group E–

Most of the plants bearing the name of *Ulothrix flaccidus* Kütz.[7] (Fig. 4) in herbarium material studied were tumid specimens of *U. subtilis* (Group C). A few plants, under various labels, seemed distinct and were later compared with field material collected from terrestrial sites. The range of diameters observed was about 6.5-10μ.

The plants are terrestrial. Filaments break readily. The chloroplast averages a flattened plate occupying much of the visible cell surface, but varies from a small lump through single and double folds. Microscopically, the color is yellowish green, especially if the plant is flooded with water for a few days. A single pyrenoid is present, and as has been noted by Hazen, it is frequently prominent. Cell length .66-2.5 times diameter. Reproduction by fragmentation only was observed.

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[7] *Stichococcus flaccidum* (Kütz.) Gay (ex-Hazen) and *Hormidium flaccidum* A. Braun (ex Heering).

Fig. 1-8. Species of Ulothrix, x 1000.
- Fig. 1. Ulothrix bacillaris (Naeg.) comb. nov.
- Fig. 2. Ulothrix subtilissima Rab.
- Fig. 3. Ulothrix subtilis Kütz.
- Fig. 4. Ulothrix flaccida Kütz.
- Fig. 5. Ulothrix rivularis Kütz. showing rhizoids and gelatinous area around break in filament.
- Fig. 6. Ulothrix tenerrima Kütz.
- Fig. 7. Ulothrix aequalis Kütz.
- Fig. .8. Ulothrix zonata (Webber and Mohr) Kütz.

There is certainly the possibility that this group represents nothing more than an ecological segregate of Group D, but attempts to grow both in a standard liquid medium were not successful in indicating a convergence of character in similar environment.

Group F–

The size range thus far observed for this group (about 6.5-10μ) corresponds well with the range of groups D and E, but there are characters of chloroplast structure, reproduction, and ecology that would seem to separate it. The separation often cannot be made in dried material, and may be impossible to make in some living specimens. *Ulothrix tenerrima* Kütz. (Fig. 6) corresponds best with the group characters, and *Uronema gigas* Vischer (culture isolated by Vischer) appears to be inseparable from it.

It is found in aquatic or dripping habitats with relatively warm water. The chloroplast, especially in young filaments may be hourglass shaped or consist of two or three pieces. In longer cells, the chloroplast may occupy only one side of the visible cell surface, while shorter cells are typically filled with a flattened plate, or a considerably curved plate displaying one or two folded edges. Occasionally, an almost complete ring is present. Some yellow tinge may be present microscopically and macroscopically. A single pyrenoid is recorded in most references to the species, but two or three are sometimes present in the largest plants which might belong in the group. Cell length .66-2.5 times their diameter. Reproduction is by motile cells. Holdfast cells are frequent, and the distal ends of the germlings are quite sharp-pointed, frequently with the uneven taper described for *Uronema* spp.

Group G–

It should be reemphasized here that there is often no absolute separation in size or other character between groups. The dividing line of 10.5μ in diameter between groups F and G fits most cases, but by no means all of them. Specimens were found ranging in size from 8-12μ and 9-13μ. One described species of a similar range is *Ulothrix moniliformis* Kütz. which is apparently based on material which was not in a normal vegetative condition. Plants were found in the range in "normal" condition, i.e., with parallel sides, thin walls, and clear chloroplasts, and they are regarded as intermediates between groups F and G. Possibly, too, plants of this size were the material described as *Uronema indicum* Mitra, but no specimens of this species

have been obtainable, and the available drawings are insufficient for definite judgment.

Ulothrix parietina Kütz. is within the size range of group G, but seems to be a distinct species, because of its terrestrial habit, and branched or multiseriate filaments.

In the group G population, mostly limited by diameter 11-17μ, the characters which have been used to distinguish *Ulothrix oscillarina* Kütz. (11μ in diameter according to Hazen, with short cells) intergrade with those of *Ulothrix aequalis* Kütz., 13-16μ in diameter (-18μ ex Heering) with cell length 1-2 times their diameter. A third species described in this group, *Ulothrix cylindricum* Prescott is 11-12μ in diameter and has long cells, 2.25-3 times their diameter in length. Longer celled individuals may intergrade perfectly with the remainder of the population, but, thus far, only one plant has been observed in this cell length range (cells 1-3 diameters long), so the species is retained here as a variety[8] of *U. aequalis* (Fig. 7), the characters of which best represent the group. The plants are aquatic, in still or flowing waters. Chloroplasts range from a flattened plate through single or double fold types to almost complete circles. Chloroplasts tend to occupy most of visible cell surface. No more than three pyrenoids are generally present. Cell length is .66 to about twice their diameter. Reproduction is by motile cells, but germlings were not observed during the study.

Group H—

The specimens whose diameters fall mostly within the limits of 17.5-25μ belong to this group, and correspond closest to *Ulothrix tenuissima* Kütz. Although both upper and lower limits are most hazy here, there does seem to be a higher frequency of occurrence of specimens measuring within the limits of the group than on its borderlines, so an entity is being recognized for it. Inasmuch as *U. tenuissima* is the type species of the genus, it is well that it may be retained.

The plants are aquatic. Chloroplasts are most frequently double folded, but vary from a flattened plate to an almost complete ring. Several pyrenoids can be present. Cell walls may be somewhat thickened, and laminated. Cells are .66-2 times their diameter in length. Reproduction is by motile cells, and germlings have well developed holdfasts and rounded distal ends.

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[8]*Ulothrix aequalis* v. **cylindricum** (Prescott) comb. nov., based on *Ulothrix cylindrica* of Prescott, G. W. 1944. New species and varieties of Wisconsin algae. Farlowia, 1:347-385.

Group I—

All specimens over 25μ in diameter were assigned to *Ulothrix zonata* (Webber & Mohr) Kützing (Fig. 8). These plants are aquatic, found in field studies in large, cold, rapidly flowing streams. The chloroplast is a broad, flattened or folded plate or somewhat thinner ring. Several pyrenoids may be present. The cell walls may be thickened and laminated. The cells are .66-2 times the diameter in length. Reproduction is by motile cells, and germlings have well-developed holdfasts and rounded distal ends.

Conclusions

1. *Geminella* Turpin (1828).

This is a rather well defined genus, its filaments separated from *Ulothrix* by their firm, gelatinous sheath. A gradation is found between unbroken filaments of cells, through degeneration of outside cell walls and formation of separate cells internally, to filaments of separate cells within a gelatinous sheath. However, the last stage has not been observed in *Geminella minor* (Naeg.) Heering.

The fresh water species of the genus observed were *G. minor* (Naeg.) Heering (with cells 3-6μ in diameter rather than 4-8μ, as described) *Geminella interrupta* (Turp.) Lag. (cells 5.5-8μ in diameter, with a distinct tendency toward separation), and *Geminella mutabilis* (Bréb.) Wille (cells 9-13μ in diameter, and separated). The specimens considered corresponded reasonably well to established classification, as found in Heering (1914) or Prescott (1951).

A collection of the marine *Geminella scalariformis* forma *marina* G. S. West was studied. As described, its cells differ from those of the other species of the genus by being wider than long, disciform. It was observed that dividing cells of this form exhibit a line of juncture such as is figured for *Radiofilum*, a genus not considered in this investigation.

2. *Uronema* Lagerheim (1887)

All available specimens and figures of *Uronema confervicola* Lag. (ex Collins or Heering) were studied and evaluated without finding convincing evidence that any of the drawings of specimens correspond to the plant originally described. This was a plant described as a short filament with holdfast, asymetrically pointed distal ends, and quite long-proportioned cells. It reproduced by zoospores. Germlings of sharp-ended *Ulothrix* spp. frequently have been named *Uronema*.

3. *Hormidium* Kützing (1843) and *Stichococcus* Naegeli (1847).

The weight of evidence is against retention of these genera separately from *Ulothrix*.

A high degree of intergradation of chloroplast types exists. Extremes of the types are easily differentiated, but they grade into forms which look quite similar. The presence of a pyrenoid cannot be used for separation of genera since all types may display these structures.

Reproduction by motile cells does not, alone, appear to be an adequate basis of generic separation here. Furthermore, the extent to which motile cells are produced in reproduction varies. The larger forms (Groups G, H) usually do not reproduce by motile spores; the smallest (group A) apparently do not. In the middle ranges reproduction by both motile cells and immotile vegetative cells are found in varying proportion. In group B frequent reproduction by both means is likely. Group F resembles the larger forms of *Ulothrix*, and *Stigeoclonium*, in reproducing mostly by motile cells. Conversely, reproduction is almost completely by non-motile vegetative cells in groups C, D and E.

The tendency of filaments to disassociate so that a unicellular condition is maintained is found to a marked degree only in one species, but since normal filaments are also produced there, the character is not considered worthy of generic status.

4. *Ulothrix* Kützing (1833)

A—the fresh water and terrestrial population.

There are few clear-cut taxonomic entities in this population. As already stated, intergradation of characters is common. While the population may be divided into groups based on diameter, chloroplast form, and reproduction, the system derived from this study is necessarily somewhat arbitrary, and occasional specimens belonging to the genus cannot be successfully assigned to a species. The species key presented here should differentiate almost all plants however, and serve as a step toward a more complete taxonomic understanding of the population.

Key to the fresh water and terrestrial species

1. Mature filaments over 25μ in diameter, chloroplast complete parietal ring or broad folded band .... *Ulothrix zonata* (Webber & Mohr) Kütz.
1. Filaments smaller 2
   2. Filaments 17.5-25μ in diameter, if varying under 17.5μ then major part of range over 17.5μ; chloroplast complete ring or

broad folded band .... *Ulothrix tenuissima* Kütz.

2. Filaments smaller than 17.5μ in diameter or, if varying over that size, then with major portion of range below 17.5μ 3

3. Filaments frequently with bilateral arrangement of cells or short multicellular branches; chloroplast usually double fold type; growing in terrestrial environment .... *Ulothrix parietina* Kütz.

3. Filaments never with bilateral arrangment of cells or multicellular branches, although protrusions may be present from single cells; chloroplast flat or folded; growing in aquatic or terrestrial environment 4

4. Filaments 11-17.5μ in diameter or major portion of variation within this range; chloroplast usually double fold type 5

4. Filaments smaller 6

5. Length of cells up to twice their diameter . *Ulothrix aequalis* Kütz.

5. Length of cells 2.25-3 times their diameter ....
.... *Ulothrix aequalis* v. *cylindricum* (Prescott) comb. nov.

6. Filaments frequently exhibiting broad range in size variation, between 5 and 11μ in diameter; short, unicellular, elbow-like or bulging "rhizoids" sometimes developed; parietal chloroplast variable, usually occupying less that half of cell surface, or flat with a single marginal fold, infrequent cells with a double fold .... *Ulothrix rivularis* Kütz.

6. Filaments otherwise, with more restricted size range, no rhizoids 7

7. Filaments 6.5μ or larger in diameter 8

7. Filaments smaller, for major part of variation 9

8. Plants aquatic, frequently exhibiting double folded chloroplast; germlings with sharp pointed distal ends, holdfasts at promixal ends; long well-developed stable filaments ....
.... *Ulothrix tenerrima* Kütz.

8. Plants terrestrial; chloroplasts usually not exhibiting distinct double folds, usually flat, or with one fold, yellowish; filaments frequently breaking or forming layer .. *Ulothrix flaccida* Kütz.

9. Filaments chiefly 5-6μ in diameter; chloroplasts usually flat, sometimes folded .... *Ulothrix subtilis* Kütz.

9. Filaments smaller than 5μ in diameter 10

10. Filaments or individual cells chiefly 2.5-3.5μ in diameter; chloroplast flat or slightly folded, usually occupying much of cell surface .... *U. bacillaris* (Naeg.) comb. nov.

10. Filaments 4-5μ in diameter, chloroplast one sided, flat, or folded, usually occupying less than .66 of cell surface ....
.... *U. subtilissima* Rab.

FIGURE 9

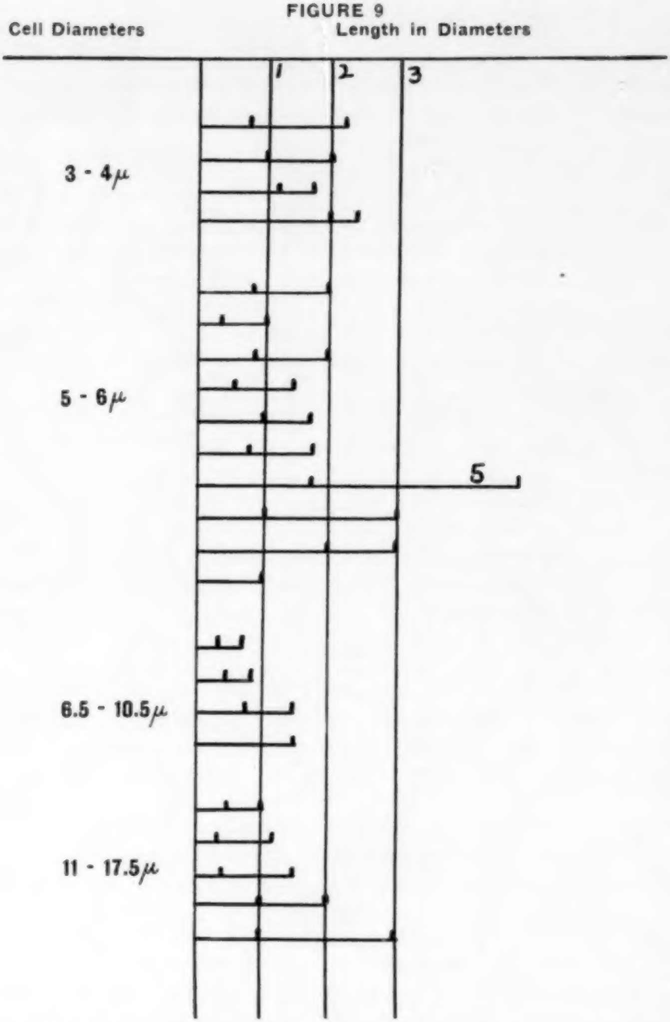

Fig. 9. Cell lengths found in various filaments of *Ulothrix* expressed diameters. Shortest and longest cells in filaments indicated

B—The marine population

Only dried specimens were available for examination, so the conclusions offered here might be altered if field studies were added. However, the marine *Ulothrix* do not seem to present the complexities encountered in the non-marine population.

Ulothrix flacca (Dillw.) Thuret and *Ulothrix implexa* (Kütz.) Kütz. are reasonably well defined in the marine population, and their published descriptions, already accepted by Hazen and many others, are agreed to here, for the most part. However, the two may not be entirely separable in specimens of about 15μ in diameter.

Specimens labeled *Ulothrix pseudoflacca* Wille failed to furnish any basis for separation from the two forms above, and it is agreed with Hazen that there is insufficient evidence for the recognition of such a species.

The branching marine species, *Ulothrix laetevirens* (Kütz.) Col. appears to exist as a distinct entity, and to have been described correctly.

It was impossible, in dried specimens at least, to distinguish *Stichococcus marinus* (Wille) Hazen from *Ulothrix subtilis* Kütz. The plants so labeled may even belong to *Ulothrix Dotyi* Silva (Silva, 1953), a small species which displays rhizoid development and branching in the basal portion.

Summary

The population of a portion of the algal family Ulotrichaceae was investigated with the view of obtaining a clearer concept of the systematics of the group than exists at present. Field collections, cultures, and exsiccatae were utilized to obtain as broad a base as possible. Cell diameter, cell length, tumidity, wall thickness,, reproduction, and the structure of the chloroplast were recorded.

The intergradation of traits encountered in non-marine *Ulothrix* indicated that this population is a complex one, and, although few sharply distinct entities could be isolated, a classification has been derived, including species often assigned to *Stichococcus, Hormidium,* and *Uronema.* Ten species and one variety were recognized.

Four species were recognized in marine *Ulothrix,* and four species of *Geminella* were found to conform to established species concepts.

LITERATURE CITED

Collins, F. S. 1909. The Green Algae of North America. Tufts College Studies, Sci. Ser. 2: 79-480.

Gay, E. 1891. Recherches sur dé developpement et la classification de quelques algues vertes. Paris.

Hazen, T. E. 1902. The Ulothricaceae and Chaetophoraceae of the United States. Mem. Torr. Bot. Club. 11: 135-250.
Heering W. 1914. Ulotrichales, Microsporales, Oedogoniales. In A. Pascher, Die Süszwasserflora Deutschlands, Oesterreichs und rer Schweiz. Heft 6. Chlorophyceae 3. Jena.
Klebs, G. 1896. Die Bedingungen de Fortpflanzung bei einige Algen und Pilzen. Jena.
Kützing, F. T. 1833. Uber eine neue Gattung der Confervacaceen. Flora, 16: 513-528.
1843. Phycologia generalis, oder Anatomie Physiology und Systemkunde der Tange. Leipzig.
Lagerheim, G. 1887. Note sur l'Uronema, noveau genre des algues d'eau douce de l'ordre des Chlorozoosporacees. Malpighia. 1: 517-525.
Naegeli, C. W. 1847. Gattungen einzelligen Algen, physiologische und systematische bearbeit. Zurich.
Prescott, G. W. 1944. New species and varieties of Wisconsin algae. Farlowia. 1: 347-385.
1951. Algae of the Western Great Lakes Area. Cranbrook Institute of Science Bulletin No. 31. Bloomfield Hills, Mich.
Silva, H. 1953. Fifteen New Algae. Bull. Torr. Bot. Club. 63: 342-348.
Smith, G. M. 1933, 1950. The Fresh-Water Algae of the United States. New York.
Turpin, P. J. 1828. Apercu organographique sur le nombre deux. Mém. Mus. d'Hist. Nat. par les Prof. de cet establissement. 16: 296-344.

The Hart's Tongue—An Interesting Fern*

Jesse F. Clovis

One of our most unusual ferns is also one of our rarest, *Phyllitis scolopendrium* (L.) Newman var. *americana* Fernald, locally known as the harts' tongue, or deer's tongue. This fern was only a name to me until the past year, when I chanced to make its acquaintance. It is a "cluster" fern, with many simple, unlobed (basally cordate-auriculate) fronds arising from a short caudex, and from a distance superficially resembles a winter rosette of some biennial weed. The dorsally located sori are linear and at right angles to the rachis.

(See the accompanying photograph, Fig. 1, taken at Clark reservation, Jamesville, New York.)

The fern was first mentioned in known literature in the "Flora Americae Septentrionalis",[1] written by Frederich Pursh and published in 1814. Pursh's "Journal"[2] describes his original discovery of the fern, on Squire Geddes' farm near Split Rock, five miles south west of (now) Syracuse), New York, as follows: "July 20 (1807) . . . . . and what I thought the most of, Asplenium Scolopendrium. This fern, which I don't find mentioned by anyone to grow in America, I always had a notion to be here; and indeed I was quite enjoyed to find my prejudice so well founded in truth. It appears to be the same as the European only smaller; is the European auriculated at the base, like this species?" (yes).

*The author wishes to thank Dr. L. C. Petry, Dr. W. C. Muenscher and Dr. R. T. Clausen for their assistance.

Fig. 1. Phyllitis scolopendrium.

In Pursh's Flora, he calls the fern *Scolopendrium officinarum* Willd., with the following notation ". . . . This species I have seen in no other place than that here mentioned (the Geddes Farm), neither have I had any information of its having been found in any other part of North America."

It might be said that Pursh had made several trips to the Geddes farm before succeeding in finding the Squire home, so that the

recording of this fern was held in the balance, until the Squire could take Pursh to it. We wonder who might have recorded it, had the Squire continued to be away, as it has since been found in several locations in the central New York area.

The "Flora of New York State", by John Torrey,[3] (1843) says the fern is undoubtedly indigenous in central New York, ". . . which is the only place where it has hitherto been found in North America. It was first detected in North America by Pursh . . . "

Asa Gray's "Manual of Botany" (1848)[4] gives the original situation, and adds a note that "it would seem" that it appears also near Canandaigua, in western New York. This last statement is credited to "Nuttall", but I am unable to secure Nuttall's Journal to investigate the reference. Canandaigua is some 70 miles west of Syracuse, and since no other reference to this location was discovered, it would seem that Nuttall was either incorrect in his location, or the station was rather quickly eliminated.

In Paine's "Catalog of Plants found in Oneida County (N.Y.)",[5] we find a reference to the discovery of another station for *Phyllitis*, this one in "Canada West", at Lake Simcoe, Simcoe County, Ontario. However, Paine still lists only the Central New York station for the United States. The "Lake Simcoe" find was possibly the one made by Professor William Hinks of Toronto in 1857,[6] and was actually along falls of the Sydenham River, which empties into Owen Sound, approximately 50 miles west of Lake Simcoe. However, the assumption that the above locations are the same is correlation on my part, and may not be correct.

In 1882, a gardener in Woodstock, New Brunswick, found some specimens of *Phyllitis*, but it is believed that this station is completely eradicated at present.[7]

The Tennessee stations for *Phyllitis* were in Roane and Marion counties, and were found in 1849 by Dr. A. Gattinger, and in 1879 by Major Cheatham, respectively.[8]

Other stations have been proposed for the hart's tongue fern, but unfortunately, these have not been supported by specimens, hence their authenticity is unproved. At any rate, its unique distribution in North America is very interesting, especially if compared with the typical European species, which seems to be prevalent throughout Europe; however, two major likenesses are seen between the typical and the variety; (1) the substrate is usually limestone or a derivative, and (2) the situation is usually cool, damp and shaded.

This short note on *Phyllitis* is, of course, extremely sketchy, and was so meant to be. Persons interested in further information are

referred to (the appended bibliography, especially W. R. Maxon's paper in "Fernwort Papers", and to) Dr. Mildred E. Faust, Department of Botany, Syracuse University, Sycracuse, New York, who I understand is compiling a rather comprehensive work on *Phyllitis*. Dr. L. C. Petry, Botany Department, Cornell University, Ithaca, New York, has considerable information on the subject, and Dr. James H. Soper, Botany Department, University of Toronto, Toronto, Canada, has done some work on the Canadian aspects.

BIBLIOGRAPHY

1. Pursh, Frederick. Flora Americae Septentrionalis. London. 1814.
2. ———. Journal of a Botanical Excursion in the Northeastern parts of the States of Pennsylvania and New York during the year 1807. Edited by Rev. W. M. Beauchamp, for the Onondaga Co. Historical Association, Syracuse, N.Y. 1923
3. Torrey, John. Flora of New York State. Natural History of New York. Albany, N.Y. 1843.
4. Gray, Asa. Manual of Botany. James Monroe & Co., Boston. 1848.
5. Paine, John A., Jr. Catalog of Plants found in Oneida County (N.Y.) and Vicinity. Regents Report of the University of the State of New York. Albany. 1865.
6. Maxon, William R. On the Occurrence of the Hart's Tongue in America. "Fernwort Papers, presented at a Second Meeting of Fern Students, held in in New York City, June 27, 1900, under the auspices of the Linnean Fern Chapter."
7. Nichols, George G. In personal correspondence to Dr. L. C. Petry. November 13, 1919.
8. Shaver, Jesse M. Some notes on Tennessee Cliffbrakes,, Chainferns, and the American Hartstongue. Tenn. Acad. Sci. Jour. 20(2): 174-202. 1945.

NOTES and NEWS

A CORRECTION ON "SPHAGNUM BOGS"—*In the course of assembling* information for a forthcoming State Geological Survey bulletin on the geology of Watoga and Droop Mountain Battlefield State Parks, Dr. Earl L. Core kindly gave me a number of references on the sphagnum bogs of that area. One of these was "Some Stages in the Development of Sphagnum Bogs in West Virginia" by G. R. Rigg and P. D. Strausbaugh which was published in the December 1949 issue of *Castanea*. In this article on page 134 the statement is made "Droop Mountain is a massive accumulation of White Medina Sandstone . . .", while on page 140 there is a conflicting statement that "The simplest type of persistent depression is illustrated by those in which Big Droop and Little Droop are developing. Here only one kind of rock is involved. The depressions are in the very resistant Pocono Sandstone . . .".

Neither of these correlations of the sandstone on Droop Mountain is correct according to the Pocahontas County Report published by the State Geological Survey in 1929. In that volume Dr. Paul H. Price identified the Droop sandstone as a member of the Bluefield group of the Mauch Chunk Series. It was originally named by David B. Reger in the Survey report on Mercer, Monroe, and Summers Counties of 1926 and Droop Mountain was given as the type locality. The Pocono of earliest Mississippian age is approximately 200 feet lower stratigraphically than the Droop sandstone while the White Medina of Silurian age is still lower by at least an additional 10,000 feet.

This is written with the hope of preventing any misconception of the geology of this well-known and intensely interesting area from being continued in scientific literature through reference to the article cited. I suspect that geologists might be similarly confused if forced to include plant identifications in their general reports.—JOHN C. LUDLUM, WEST VIRGINIA UNIVERSITY.

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